Art. VIII.—*Outlines of a System of Mechanical Philosophy, being a Research into the Laws of Force.* By Samuel Elliott Coues. Boston: Little & Brown. 1851. 12mo. pp. 330.

If a teacher, or a professed student, of the science of mechanics, or even a person who has the ordinary knowledge of this science which every educated man is supposed to possess, were told that this very original and striking book contained an elaborate attempt to disprove the Newtonian doctrine of gravitation, and to show that all the explanations which it offers of the phenomena of the physical universe are unsatisfactory and unsound, he would be likely to throw aside the volume unread, and with an exclamation of impatience at the perverted ingenuity manifested by sciolists in assailing the fundamental truths of science, which have been a thousand times so fully demonstrated, that reasoning against them could only proceed from ignorance or a morbid love of sophistry and skepticism. Yet we believe that his determination would be a hasty and injudicious one, not merely because it would deprive him of the pleasure he might receive from a highly entertaining book, not less remarkable for its literary merits than for the novel and comprehensive views which it presents of the phenomena of nature, but because it was probably dictated by an imperfect and unphilosophical conception of the true character of Newton's doctrine, of the aid which it renders to mathematical calculation, and of the sort of explanation which it affords of the wonders of the outward universe. The usefulness of the Newtonian theory, considered simply as a theory, or as a means of grouping physical facts together and viewing them as a whole, and of applying numerical calculation to them, is entirely independent of its truth when considered as an exact explanation of the *cause* of the phenomena. If this theory should be finally disproved, by discovering an incongruous fact or devising a crucial experiment, it would remain, in reference to all the phenomena to which it is now applied, just as useful and trustworthy a means of computation as ever; it would enable us to predict with equal certainty and precision the eclipses and other occultations, the paths of the comets and their periods of re-

turn, and all other recurring phenomena of the heavens. It would retain this valuable property, because, as an organon or method of inquiry, its function is only to bring together, and to enable us to deduce from one principle, the observed facts which are the real basis of prediction, or data from which the calculation is made.

Kepler's laws are the largest generalization that has been formed of observed facts in astronomical science ; and these laws, when first made known, were strictly empirical, having been discovered by sifting the facts, and ascertaining that they could be distributed into a few classes, according to their obvious analogies and relations. The correctness of these laws, consequently, can never be impeached ; they represent correctly the facts of observation, and are properly nothing but an abridged statement of these facts. Newton's theory of gravity carries the generalization one step farther ; it brings together Kepler's three laws, and one other general fact of observation, or empirical law, — namely, the manner in which a stone thrown by the hand ultimately comes to the ground,— by deducing all four of them from one supposed or imaginary principle ; that is, from the *hypothesis*, that all aggregations of matter attract each other in the direct ratio of their masses, and the inverse ratio of the squares of their distances. Newton's theory, then, is not an empirical law, but a hypothetical or imaginary one. He does not say that this attractive force exists, but only that all bodies move or rest *as if* it existed. His hypothesis, in regard to the solar system, amounts to the same thing as if he had imagined, that all the planets are bound to the sun and to each other, and all satellites to their primaries, by elastic, material ties, the strength of which varies directly as the masses of the bodies which they connect, and inversely according to the squares of their own length.

But the theory of gravitation alone is not enough to explain the phenomena. We must also *suppose* that a heavenly body, — the moon, for instance, — was originally launched in space with a projectile force sufficient to carry it entirely round its primary before the attraction of gravity could bring it down into contact with that primary ; and, also, that the motion thus communicated, or any motion proceeding from a single impulse, will be prolonged in a straight line, with a uniform velocity, forever. With these three convenient

mathematical fictions, we can construct a system which shall embrace the mechanism of the heavens, and furnish inferences that will explain, more or less perfectly, many facts of observation on the surface of our own planet. In what light Newton himself regarded the chief of these hypotheses appears clearly enough from the letter to Dr. Bentley, in which he says, "that gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance, through a vacuum, without the mediation of any thing else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man, who has in philosophical matters a competent faculty of thinking, can ever fall into it." This language is not too strong, for the truth of the hypothesis is literally inconceivable. Brute matter cannot act where it is not; for action is a mode of being, so that to say, 'a body can *act* where it is not,' is equivalent to saying that 'a body can *be* where it is not.' How, then, can the sun act upon our earth, which is 95,000,000 of miles distant from it? Newton meant only that the earth revolves round the sun *as if* attracted by it, or bound to it by an elastic, material tie, under the conditions specified.

The postulate, moreover, that the natural path of motion is a straight line, is not supported by experience, and not susceptible of proof. Nearly all the natural motions with which we are acquainted are curvilinear; even the stone which flies from the sling enters immediately upon the curved path which brings it ultimately to the ground. If a planet, in like manner, were suddenly released from its centripetal force, we have no *proof* but what is derived from the *postulates* of the Newtonian system, that it would fly off in a straight line rather than assume a new orbit of revolution. The ancients believed natural motion to be curvilinear; the moderns have reversed this doctrine, simply because another of their assumptions — that all bodies attract each other — rendered this second postulate necessary. As was remarked on another occasion, "Newton found that the elliptical motions of the planets could not be mathematically represented by the hypothesis of one mechanical force operating on them constantly and uniformly; and so he *imagined* two forces, one being that of gravitation, which tends constantly towards

the sun, and another by which they tend to fly off at a tangent from their orbits; or the latter may be considered as the result of the primitive projectile force with which the planets were originally launched in space. From these convenient fictions, he found he could deduce, mathematically, their true motions. It is possible, though certainly not probable, that some mathematical theory will hereafter be invented, which will account for the motions of the system on the hypothesis of a single force; if so, it will immediately take the place of the present theory, on account, not of its superior truth, but of its greater simplicity."

And this is precisely what Mr. Coues has attempted to accomplish in the work before us.

> "The law of gravitation," he says, "is not needed for the motion of the heavenly bodies. There is more truth to nature, there is more simplicity and beauty, in the idea that the force of the revolving body is within itself; that its curvilinear motion is its natural motion; that it goes round in its orbit without needing the guidance and direction of central and tangential forces; that it can be trusted to the unerring energy imparted to it from the beginning. It needs no great presumption thus to affirm; for by ancient philosophers, and in more recent times by Copernicus, Galileo, Kepler; Des Cartes, it was believed that circular motion was the natural motion." p. 81.

The undertaking of Mr. Coues is, therefore, a daring, perhaps even a presumptuous, one; but it is not to be immediately dismissed merely because it conflicts with the Newtonian theory, which has a great body of mathematical evidence in its favor, and has so preoccupied the ground that there is no room for a rival hypothesis. It is to be tried on its own merits, quite irrespective of the brilliant conformity of astronomical calculations with the observed results, which, in common minds, is the great support of the Newtonian system. Strictly speaking, mathematical science can offer no proof whatever of a physical fact; it can prove nothing but abstract propositions. When applied in the mixed sciences, it simply enables us to make a more strict and exact comparison than would otherwise be possible of the results of theory with the facts of nature. The only test of any hypothesis respecting the true cause of certain phenomena is observation and experiment; and a competent knowledge of

mathematics will enable us to apply this test with the utmost precision. With it, we can calculate, to a hair's breadth, the necessary results according to theory; and then, with the immense improvements of modern times in the instruments of observation, we can determine with equal accuracy the character and limits of the phenomenon. The astronomer, in his observatory, can determine the time at which the occultation *did* take place, within the tenth part of a second; and the mathematician, in the room below, can fix the time when, according to theory, it *ought* to take place, within the hundredth part of a second. The nice coincidence thus made out affects us with wonder, and seems to common minds a mathematical, and therefore incontrovertible, proof of the truth of the theory. But the coincidence itself can be made out, in a rough way, with the naked eye as the only means of observation, and by a train of reasoning from the theory so consequent and direct, that a mind of great analytical power could follow it without the use of one mathematical symbol. And the coincidence itself, whether roughly or nicely determined, affords just as much proof of the theory as would be gained in favor of any hypothesis as to the manner in which my neighbor's house caught fire, by showing, experimentally, that another house might be so fired under precisely similar circumstances.

The presumption in favor of the Newtonian system, which leads us to regard with so much suspicion and dislike any new theory that is devised to take its place, is founded, not so much on the nice coincidence between prediction and observation in astronomy, as on the great multitude of physical phenomena on the surface of our earth, of which the system offers an intelligible and satisfactory explanation. That the theory tallies with fact in relation to the motions of the heavenly bodies is a consideration which loses much of its importance when we remember, that such masses or densities must first be assigned to these bodies as will suit the theory; that the masses thus assigned cannot be directly verified; and thus, having formed what is in part a hypothetical conception of the solar system, we find that this system in all its parts will square with our hypothesis. As Mr. Whewell says, "the form in which the question of the truth of the doctrine of universal gravitation now offers itself to the mind of astro-

nomers is this: that it is taken for granted that it will account for the motion of the heavenly bodies; and the question is, with what supposed masses it will give the best account." The density which we are thus obliged to assign to the planets varies by no regular law, but seems to be distributed fortuitously. Mercury is more than three times as dense as the earth; Venus and Mars are both somewhat less dense than the earth, which lies between them. Jupiter has less than one fourth of the density of the earth; Saturn is but little more than half as dense as Jupiter, while Uranus is a little more dense than Jupiter. There is no uniformity apparent here; and accordingly, we are not surprised to learn that the calculations recently showed the law of Jupiter's attraction upon the asteroids to be somewhat different from what it is upon his own satellites; — a difficulty which was met by a new determination of the mass of Jupiter by Mr. Airy. As the density of the attracting bodies is always one element in the calculation, we may say that the only verification of the Newtonian theory which is afforded by astronomical science depends upon the previous assumption that this theory is correct. We do not say that the argument, even thus understood, has no validity whatever. The density which is necessarily assumed the first time the planet's attractive power is calculated, is found — perhaps with some trouble and after numerous corrections in the process — to answer for other cases, perfectly distinct from the former one; in other words, the determination of the mass from its action on the nearest planet is found to agree very well with its determination from the action of the planet upon its own satellites. Such coincidences, which are neither very numerous nor very exact, form about the only argument which can be gleaned from the computations of the astronomer for the truth of the theory of gravitation.

The theory, as we have intimated, is far more securely buttressed by the many plausible explanations, which have been deduced from it, of perpetually recurring phenomena upon the surface of our own planet, where, the substances exhibiting the phenomena being generally within our reach and control, experiment can be added to observation as a means of research. Here, also, the facts must sometimes be modified to suit the theory, the truth of the latter being pre-

viously assumed. Thus, in order to determine the density of the earth, Dr. Maskelyne made a series of experiments from which it appeared, that the mountain Shehallien in Scotland causes a lateral deflection of the plumb-line which is hung near its base, amounting to nearly six seconds. But M. Bouguer found that Chimborazo, a mountain vastly larger than Shehallien, produced a deflection of less than eight seconds. This being much less than what theory required, the difficulty was got over by supposing Chimborazo to be hollow from the effects of volcanic action. In the celebrated Cavendish experiment, recently repeated by Mr. Baily, the attraction of two large masses of lead for two light balls of different substances was rigidly determined through the delicate torsion balance, and the density of the earth thus ascertained agrees very well with the result of Dr. Maskelyne's experiment. Mr. Coues, we may remark in passing, does not notice the Cavendish experiment, the inference from which seems to be irreconcilable with the truth of his hypothesis.

Generally, however, a great number of facts and experiments, which do not need to have Chimborazan difficulties sifted out of them by conjectures that cannot be verified, are detailed in the common treatises on mechanics, the explanation of which upon the principles of the theory of gravitation appears very simple and plausible. So far as the reasoning which connects them with these principles is sound, they are coincidences of fact with theory. The argument for the truth of a theory, which is afforded by such coincidences, is not irrefragable; a rival theory may be propounded, which shall also account for the facts, and in a manner still more simple and satisfactory. But the argument is strong; the probability created by it, in this instance, is very great. The Newtonian system has occupied the ground for a hundred and fifty years, during which the spirit of inquiry, and even of skepticism, has been rife; and it has never been seriously shaken. A vast multitude of phenomena have been explained in accordance with its principles, and distributed in conformity with its laws. One difficulty after another in the way of its application to nature has been removed by the patient assiduity of scientific calculation and research, and by the vast improvements which have been effected in the modes of observation. The *probability* in its favor, we say, is very great;

but there is still a double *possibility* of error. The reasoning which deduces the facts from the theory may be shown to be unsound; or, if this cannot be done, another hypothesis may be started, which shall explain the facts equally well, or better. The logician, who has repeatedly tested every link in the chain of ratiocination, may laugh at the former possibility; but he cannot peremptorily rule the other out of court without examination. He is bound to give a reason for the ancient faith to which he adheres, by demonstrating the insufficiency of the novel explanations that are offered.

Our readers must not suspect us of an intention to emulate the daring of Mr. Coues, by openly assailing the Newtonian doctrine of gravitation, and offering another hypothesis to take its place. We have simply endeavored, with a primary reference to the logic of the physical sciences, to indicate the point of view from which such an attempt should be judged, and the nature of the prejudice by which, if such an hypothesis ever should be admitted to dethrone its rival, its early reception would certainly be obstructed. We have sometimes suspected, that there is a *vis inertiæ* in the minds of scientific men, as well as in brute matter. It is very difficult to start them from a position they have long occupied; but when once in motion, they roll on without much impediment, and often with an accelerated velocity. With regard to the performance of Mr. Coues, which has suggested these remarks, we can only say, that it is a work from which any reader may derive much entertainment and instruction, whatever opinion he may ultimately form as to the truth of the system of mechanical philosophy which it expounds and advocates. In the attractive qualities of its style, in descriptive power, in the skill with which the novel doctrine is propounded, and in the ingenuity and richness of the illustrations that are heaped around it, we have been often reminded of that noteworthy and notorious book, "The Vestiges of Creation;" though in sentiment and the tendency of doctrine, so far as religious belief is concerned, no two works could be more widely contrasted. In regard to the truth and sufficiency of this new theory, we have not formed any opinion, simply because the whole of the evidence on which it rests is not yet presented. The author of it observes in his preface, that "this work is not intended as an elaborate, phi-

losophical treatise, but as a rapid outline; and only those considerations are presented, which are necessary to develop the opinions advanced. Much collected material has been laid aside for future use, if it be thought that the opinions set forth are worthy of additional attention." We regard the work, therefore, merely as an ingenious speculation, which has led the maker of it to subject to a severe scrutiny some of the received Newtonian explanations of physical phenomena, with a view to test their soundness and to offer a new mode of accounting for the facts upon the principles of his own system. It is obvious that he may have had some success in the former part of his undertaking, though his own theory should appear on examination to be open to objections equally formidable. Some of his criticisms upon the received doctrines of mechanical science seem to us to have great weight; others are less important, or may be obviated altogether by trifling corrections of language.

The leading thought of the new system is clearly indicated in the sentence which stands as the motto on the title-page: — "That which we call gravitation, and fancy ultimate, is but one fork of a mightier stream, for which, as yet, we have no name." The doctrine of the identity of force, which is a capital feature of the work, is not, strictly speaking, a novel speculation; it has often exercised the minds of philosophers, both at an early stage in the history of science, and in our own day. But the manner in which it is here developed and applied is entirely original. We have no idea of force in the outward world, except as the unknown cause of motion, or rather of change of state. The only possible definition of it is, that which induces such change; and, consequently, that form of philosophy is most simple and intelligible, most in harmony with what we know of the general character of God's works, which traces all change, all motion, to *one* force or unknown cause, operating uniformly, or under general laws. Take away the supposition that the natural path of motion is a straight line, admit that it is curvilinear, and there is no further necessity for imagining an attraction of gravitation to account for the revolution of the planets about the sun. We then have these orbs propelled by a single force, acting under the simple law discovered by Kepler; namely, their velocities are such that the line which

connects each with the body around which it revolves, describes equal areas in equal times. The revolution of a planet round the sun is accompanied by rotation on its own axis, every atom in the system having two motions, orbital and rotary. Are these two motions subject to the same law, so that we can imagine them to be produced by a single force? At first sight, the two movements seem to have an opposite character. In rotation, the velocity diminishes as we go from the circumference to the centre; the axis is stationary, while the velocity of the surface, at the equator, is more than a thousand miles an hour. But in revolving round the primary body, the velocity increases as we go towards the centre. Still, the ratio of diminution in the former case is the same as the ratio of increase in the latter; we have the same law, as it were, exhibited under its opposite phases.

"From the nature of the rotating sphere there is one fixed ratio of increase of the velocity of its parts. Given the velocity at any distance from the centre of rotation, the velocity at any other distance can be determined. Its ratio of increase is proportional to the increase of the area of the circle described. We have the same measure of increase outward in rotation, that we have inward in revolution. The primary and secondary movements are under the same general law. Besides, there is also a fixed ratio between the velocities of both movements, for such is the far-pervading law of nature.

"If the orbit of any planet were enlarged without additional force of propulsion, it would move, in its new orbit, with decreased velocity. Its motion would not be harmonic; it would not describe the equal area of planetary motion. To bring it into harmonic motion it would require additional force, and this additional force needed would be measurable by the increased area of orbit. If, on the other hand, the orbit of any planet were decreased, with the same propelling force, its velocity would be increased, and it would be out of harmony with its associated worlds. With equal velocity it must part with, or transfer, a portion of its force. This degree of spare force would also be measurable by the reduction of the area of the orbit. The degree of force required or imparted is not measured by the increase or diminution of the circumference. The orbit does not measure velocity. It is a path of motion, continuous, without beginning or end. By a fixed law, the required force is determined by the area described, which increases and diminishes in a higher ratio than the length of the circumference which en-

closes the area. Thus, by the enlargement of a planet's orbit, it would need added force to preserve the harmony of planetary motion, and by its diminution it would impart force, and the force added, or given up, would be measurable by the change of area. In the words of Herschel: — 'The law of the areas determines the actual velocity of the revolving body at every point, or the space really run over by it in any given portion of time.' If the velocity be thus determined, so is the present force which determines the velocity.

"This is equally true of the velocity and force of rotation. If a mass at the surface of the earth be elevated, it gains thereby a superior level of rotation; its orbit is enlarged, — it requires additional force. If the mass fall, thereby decreasing its orbit of rotation, it has the spare force of descent; it requires less for rotation, and the falling body imparts force. The force received for elevation, the force given out by depression, is measured in one case by the increase, in the other by the decrease, of the area of the circle of rotation. Though in one view it appears as if there were the converse action of force, yet the general law is apparent, of the increase and diminution of required force for any area of orbit, whether acting outwardly from the centre, or inwardly to the centre, of revolution." pp. 19, 20.

Having attributed to one force all the motions which belong to the solar system, the author proceeds to inquire if all terrestrial motions and changes of state can be traced to the same cause, or brought under the same law. And first, with regard to the accelerated motion of falling bodies: —

"In the act of falling, whereby a lower level of rotation is assumed, force must be transferred from the falling body when its motion is suspended.

"The motion of falling is added to the rotary motion, thereby giving action to the present force. This motion being suspended, only rotary motion in a smaller orbit remaining, force will be transferred. Hence arises spare force of descent. The spare force of descent will be measured by the degree of descent. Hence the spare force of falling bodies is measured by the square of the time of descent, equal times giving equal distances of descent. Thus most distinctly is presented the law of falling bodies, or motion begetting motion, which, indistinctly understood, caused the great controversy between the schools of Des Cartes and Leibnitz, and which to this day has obscured mechanical science, by making the occasional abnormal motion of falling bodies the element of calculation of harmonic motion." p. 10.

The process of reasoning in the new theory, then, is the reverse of that which is followed under the Newtonian system. In the latter, the comparatively free and regular movement of the heavenly bodies in their annual orbits is deduced from the accelerated and inharmonic motion of bodies falling to the earth; in the former, "the abnormal motion is detected and measured by the normal, the motion of bodies in confined space being determined by the velocity of bodies in free space." To the obvious objection, that the spare force of descent is greater for an equal change of orbit at the equator than it is towards the poles, Mr. Coues answers, that a fall of one hundred feet at the equator changes the area of the orbit much less than an equal fall in a high latitude; "so that the increased ratio of the decrease of area compensates for the decreased force of rotation." Indeed, the spare force of descent slightly increases as we go from the equator, owing to the spheroidal form of the earth, which makes the decrease of area more rapid as the diameter of the orbit of rotation is diminished.

"There is not, then, one law of force for the revolution of the spheres, and another for their rotation, and another for the changes of level of the masses composing the spheres; and we think that we can show that the law, by which the spheres and masses are moved, also directs and governs the motion of the atoms composing every mass and sphere." p. 21.

Mr. Coues supposes that force is indestructible, and that it acts independently of direction; the phenomena which are usually referred to the law of action and reaction, he considers as indicating a simple transfer of force and a change in its direction. But as force does not always produce an apparent motion, he is obliged to suppose, that, besides the consentaneous motion of the atoms, or the progressive motion of the mass, there is an atomic or molecular motion of every atom composing the mass. Then, the transfer of force, which is not sufficient to move the whole mass, gives molecular or vibratory motion to the parts of the mass.

"Vibratory motion being the unequal or rather unconsentaneous motion of the parts of a mass, if there be not a gradual increment of motion from the surface to the centre, there will be nodes or points of rest. Thus musical chords manifest certain points of rest.

"Vibratory motion being determined by the degree of force, if the same quantity of matter be moved the same distance, the times of vibration will be the same; if the distance be diminished, there will be an increase of velocity. Tone being dependent on time of vibration, an increase or diminution of force changes the range, preserving the tone. Isochronous vibration is, therefore, a branch of the law which induces the same quantity of motion by the same quantity of force. It follows, too, that there can be no vibratory or reciprocal motion in free space." pp. 14, 15.

These principles, which, to avoid mistake, we have given mostly in the author's own language, are very simple and coherent; and if, as applied by rigid calculation to each case, they are found to describe the phenomena of nature exactly, the theory becomes exceedingly plausible. The mechanical principles of the Newtonian system are not applicable to the minute and insensible motions of the particles of matter, or to the phenomena of the imponderable agents, such as light, heat, electricity, and magnetism. We are obliged to learn a new philosophy here, and to study a whole set of novel forces, acting, for the most part, under peculiar laws. Mr. Coues, with characteristic boldness, stretches his generalization over the whole field, and undertakes to apply the principles, which he has derived from the operations of nature on the most extended scale, to the minute and intricate phenomena which are contemplated by the chemist and the electrician. He resolves all the subtle agencies which seem to constitute the *arcana* of Nature, and which, by their action on particles of insensible magnitude, produce marvellous and perplexing changes of form, into the single force which moves the spheres. But this portion of the work is less developed, the author's views being indicated only in a rapid outline, which is hardly distinct enough to show their peculiarities, or to afford material for estimating their correctness. We pass over them, therefore, in order to give our little remaining space to a consideration of the doctrines of the pendulum, the barometer, the pressure of fluids, the tides, and other phenomena, as they appear under the light reflected upon them by the two systems of mechanical philosophy which are here brought into comparison. The simple and concise statements of the book hardly admit of abridgment or of alteration, ex-

cept for the worse. We can follow the author but a little way, and that without much discussion, often adopting his phraseology, even when our limits will not allow us to copy the passage entire. The portions of the Newtonian philosophy to which he decidedly objects are briefly indicated as follows: —

"The errors of mechanical philosophy, if there are errors, arise from the assumption of gravitation, or the attractive power of matter; from the application of the law of the motion of falling bodies, to the uniform motion of bodies remaining in one determined orbit; from the belief that rectilinear motion is the natural motion, and that the curvilinear is a constrained motion, induced by conflicting forces; and from keeping out of sight the intense motion of every atom in its rotation and revolution with the earth, which, from its greater comparative velocity, supplies the governing or controlling motion, and by reference to which alone, incidental, retarded, or accelerated motion is to be understood or to be explained." p. 17.

What is usually called *momentum* is here explained to be "the degree of force present in action upon the body." It requires a greater force to move a large mass than a small one, so that the former imparts more force when its motion is arrested. Every atom in it needs its portion of force in order to be moved; and the velocity being the same, the total force is proportioned to the mass. But some time is needed for the communication of force to a mass, or, in other words, for the gradual induction of progressive and consentaneous motion through the diffusion of the force by atomic motion. Hence the slowly moving bullet will enter the water, while, if shot from a gun, it will be deflected. A column of water of the same diameter as the bullet must be pushed aside to make room for it, and time is needed for the induction of force enough into every particle of this water to give it the required motion. These simple principles seem to us to throw much light upon the *vexata questio* under the received philosophy, whether the force is proportional to the velocity, or to the square of the velocity. The same amount or energy of force may be diffused through a large mass moving slowly, as in a ship floating with the tide, or concentrated in a small mass moving swiftly, as in the bullet shot from a gun. We may need force for two purposes, — either for inducing molecular

or vibratory action in the particles of a body, as in cleaving a rock; or for inducing consentâneous and progressive motion, as by moving the rock onward. Time is needed for this latter object, in order that the force applied may have opportunity to diffuse itself among the atoms, and bring the whole bulk into forward motion; we apply the force through a large mass moving slowly, as by *prying* with a huge lever. For the former purpose, we explode gunpowder, which exerts its whole force instantaneously, or with a velocity which is immense in proportion to the mass. The received doctrine of *momentum*, then, which makes a large mass moving slowly the exact equivalent in respect to force of a small mass moving with a velocity proportionally greater, is practically untrue; and from overlooking the different occasions on which force is applied, the dispute about the relation of force to velocity has arisen.

Oscillation, or vibratory movement, Mr. Coues holds to be irreconcilable with the Newtonian theory, and to afford the strongest confirmation of the truth of his own hypothesis. "The swing of a pendulum, increasing in velocity in proportion to the decrease of distance from the centre of oscillation, the force of its motion being measurable by the area of the circle of which it describes a segment, shows that the nature of force is the same, however its energies may operate. It brings to mind the relative speed of the planets, increasing as the diameter of their orbits diminishes. Its regular beats mark time with the same precision as does the harmonic motion of the heavenly bodies." The weight of the mass moved and the length of the sweep do not change the time; give a stronger impulse, and the arc through which the vibration takes place is lengthened, but the movement is completed in the same time. The rise being equivalent to the fall, and seeming to be a continuation of that fall, — a continuation without break, — there is no apparent reason for attributing the force to a downward, rather than to an upward, attraction. Take away all impediments, let the pendulum swing in a vacuum without friction, and the vibrations will continue forever. The force of gravitation being constantly directed towards the centre of the earth, why should it not at last bring the vibrating mass to rest in the line which connects the centre of oscillation with the centre of the earth? The pertinency of this ques-

tion can be more clearly seen by taking another instance of vibration.

"In a scale-beam, balanced by two equal weights, how could oscillation take place under the law of gravitation? Yet, if you give one of these weights an impulse, they will oscillate, — oscillate forever in a vacuum without friction. Gravitation has no power to give alternate motion, — attracting both weights with equal force, it cannot first make one heavier and then the other. If both were held with equal strength, it would be absolutely impossible for this vibration to take place; there is an absence of all cause, or tendency, or capacity for oscillation. 'Vis inertiæ of motion' gives no aid; for the motion is suspended and renewed at every vibration. Nor does 'action and reaction;' for the difficulty is to account for the action. The earth might as reasonably be supposed to attract only one side of an evenly balanced wheel, and thus give it continuous rotation, as first to attract one weight and then another, when both weights are equally heavy. The motion is unquestionably from the transfer and retransfer of rotative force. The balance rotates as one mass with one degree of force, but this force flows from one part to the other of the mass.

"So far from being able to induce oscillation, the law of gravitation would immediately overcome the motion. The power of attraction, it is said, *in*creases with the decrease, and *de*creases with the increase of the distance from the centre of attraction. The weight going down is therefore more forcibly attracted, the weight going up is more feebly attracted, and this in an increasing ratio, both for the depression and for the elevation. The difference, it may be said, is so slight, that its results can never be detected by observation. But, slight as may be this want of equilibrium, it actually exists. Balances have been made so perfect and so nicely adjusted, as to turn by the impulse of the thousandth part of a grain. Suppose a perfect balance, without friction at the fulcrum, and acting in a vacuum. Here this want of equilibrium would be felt; the descending weight, being more attracted, could not rise; the ascending weight, less attracted, could not fall. There could be no oscillation under the law of gravitation." pp. 88, 89.

Some of the great waves, or "rollers," which rise occasionally in the southern ocean, show a vast disturbance of the equilibrium of nature. When millions of tons of water are thus heaped up in one volume above the ordinary level, the different action of gravity on the elevated and depressed portions must become sensible. The balance could not be restored; "it must be that the ascending water would continue to rise, and the valley of depression to sink lower." The

undulation of the water, on the new theory, is explained from the same principle as the oscillation of the pendulum; the impulse given by the wind, or by the finger in swaying the plumb-line from its perpendicular, calls out the force of rotation at different levels, a force which resides in the oscillating mass itself, and is not communicated to it from below. The length or sweep of the vibration is determined by the strength of the impulse; and the vibration is repeated, because the force liberated by the descent of one arm to a lower level of rotation raises the other arm to a higher level. The latter then descends, and the force might thus be transferred from side to side perpetually, if it were not gradually given off by friction and the attrition of the atmosphere.

"The motion of the wave is deeply interesting in all its aspects. The crests of the surge rise vertically, though they are apparently progressive. It is not until the water shoals toward the beach that they acquire a progressive motion, — progressive because near the shore there is not depth for the vertical movement. It requires double the generally supposed depth to form the vertical wave; for the uplifted water falls as much below the valley of depression as it rises above it. There is a descending wave, a current under water, which, as a wedge, forces up the succeeding wave, an opposite wave under the water. Hence on the shoaling water is the undertow, a retreat of the water at a certain depth with advancing water at the surface. The extent of this reciprocal wave is in exact proportion to the extent of the visible wave.

"Besides, the surface of the atmosphere is also to a degree in oscillation with the water over which it is spread. The wind conforms to the surface, it oscillates with the water; the sails of a boat, or the lower sails of a ship, are not becalmed with a steady wind, though surrounded by a wall of water. Says an experienced navigator, 'the lower sails in scudding are at times becalmed, from the rise of the stern of the ship, but never becalmed *on* the wind, though the waves rise higher than the sails, a fact which I have often tried to account for.' For this reason, — the undulating motion of the wind over the undulating surface of the sea, — is it that the wind off shore, not having acquired the corresponding undulation, however strong it may blow, smooths the sea.

"A most interesting scene was described by a friend, who stood on a small rocky island in the midst of the ocean, looking with intense delight on the forces of nature as displayed in a storm. The mountain waves were dark, almost black, the intensity of their gloom being heightened by the circle of white

foam which surged upon the rocks. A sudden veering of the wind, this wind being without the conforming undulation, prostrated the billows as it were at once; the ocean became as smooth as in a summer's breeze. There was no surge on the rock; but the surface of the sea was white with foam and curling in wreaths of vapor as far as the eye could reach. Gradually, however, the new wind acquired the sympathetic undulating movement, the waves again began to rise, and soon the surf dashed with its former fury against the island.

"Only because the rise and fall of the wave are from the reception and transfer of force, the mean level of the ocean ever remaining the same, could this result have been produced. If there had been 'accumulation of momentum to an enormous degree,' — if the attracting earth had drawn down the depressed waters with more strength than the elevated waters, — the ocean could never have been thus smoothed into the quiet of one level; the new wind would have increased the surging waves, so that they would have lifted their crests still higher in confused and broken masses, resulting in a conflict of motion which the strength of no vessel that ever floated could withstand." pp. 94 - 96.

If opposite winds meet, the force with which they were propelled is not destroyed by their collision, but receives a new direction, and thus creates that rotary movement of the air which constitutes a whirlwind. The force of the revolving wind is communicated to the surface of the earth beneath, and water or light bodies, having force thus imparted to them for rotation at a higher level, rise in the vortex. If a vessel be filled with water, and an orifice be then opened in the bottom to allow the water to escape, a vortex is formed in the vessel as the level of the fluid descends. The cause of this vortex is not very manifest upon the Newtonian theory, and various explanations of it have been given, none of which are satisfactory. Here is circular motion induced without a conflict of forces; for, according to the hypothesis, there is but one force present, that of gravitation, which acts directly downwards. We see this phenomenon most frequently in a tunnel, and it has been suggested that the vortex was formed by the conical shape of the tunnel; but this will not do, as the vortex may be formed in a vessel of any shape.

"The explanation of the fact is very simple on the principle which we present. The water issuing from the tube uses, in its downward motion, all the spare force of its own descent to a lower level. But the water remaining in the tunnel has also

descended to a lower level of rotation. It has therefore spare force, and this force has no other range than to confer circular motion to the water. The water therefore revolves. Thus is it, that facts, common, trivial facts, declare the general law." pp. 98, 99.

The only result to be expected from such a force as gravitation is described to be, according to our author, is, that it would draw the particles of matter more closely, and bind them more firmly, to each other. The consideration of it generally belongs to statics, not to dynamics. It is not even claimed, that it *gives* motion to the heavenly bodies; but only that it impresses upon this motion, which is due to the original impulse of projection, a peculiar character, continually bending its line of direction into a curve. So, also, upon the surface of the earth, a distinction is acknowledged between the weight of a body, or its tendency to fall, and its force of descent, which is proportioned to the time of the fall. According to the latter, it is difficult to account for the beginning of the fall, for until *some* time has elapsed, no force of descent is gathered; "when a body begins to fall from rest," says one author, "it begins to fall with *no* velocity." But according to the former, which is a constant tendency, and proportioned to the whole mass, when the support is withdrawn, the body should fall instantaneously with the entire force of its weight. Mr. Coues does not admit that there is any *weight,* properly so called, manifesting itself only as a *tendency* which never becomes a reality, or as a latent force which does not produce motion; for the only idea we have of force is, that it *does* produce motion, and we cannot recognize it in any other function. However frail the supports of a body may be, it will not crush them and fall until an impulse is given, a vibration induced, and some force consequently gathered by descent to a lower level of rotation.

"Practical men, who know only what they have seen, and believe in what takes place, without being troubled by theory, work on the faith that there is no force of gravitation in the mass which preserves one line of rotation. In taking away the foundation of a brick wall which had been undermined, to rebuild it, the owner expressed his fears that too much had been removed of the support at once, and that the building would fall. The reply of the mechanic was, 'I used to fear, but now I know that it is safe

until it *begins* to move, and I shall work without any jar; if it should begin to move, twice the present support would not keep it up.' In taking away the support of the arch of a stone bridge, after the keystone was put in, a slight, sudden sag of the bridge crushed the keystone, — a stone, the cohesion of which, if standing firm at one level, would have borne all the granite that could have been piled miles high upon it.

"This principle is evidenced by the superior security of the arch for bridges and similar structures. In the arch, there can be no descent without crushing the material of which it is formed. Thus, too, has the passing of a body of troops over a bridge, with measured step by beat of drum, often given the vibration which calls into action the weight or force of descent." pp. 107, 108.

Our author cannot admit, then, that there is any pressure of the atmosphere, or any increase of density in its lower strata proceeding from the weight of the superincumbent air. Each stratum, he argues, must have a force of rotation proportioned to its own level, and can impart force only by descent to a lower plane, where less rotary power is needed. Force is given out as it descends, additional force is needed to raise it to a higher level, where it will revolve in a larger orbit. But no force is present while it is stationary in altitude, except that which carries it round in its orbit with the earth; because no other force is manifested, and it is of the very nature — it is our idea — of force, that it must manifest itself by producing motion. The barometer, then, does not indicate altitude *indirectly*, by showing that the pressure of the atmosphere is diminished, a fact from which we infer greater elevation; but indicates it *directly*, by showing that the force of rotation is greater, just as a planet's diminished velocity indicates its greater distance from the sun. Mr. Coues argues that the increasing cold, as we ascend in the atmosphere, would condense the air as fast as the diminished weight would rarefy it; that the elasticity of the air, increasing with its density, would act against the rarer strata, and preserve the equilibrium of density; that, on account of the perfect intermobility of the particles of air, every wind that blows would tend to restore the equilibrium; and that the air, like other fluids, is supposed to press equally in all directions. Moreover, the atmosphere being a constant quantity, and thus regularly packed in horizontal layers of constantly increasing density, how can we account for the great variations in the

indications of the barometer at the same level and the same place, variations that have a range of about three inches? Mr. Coues accounts for them, on his own theory, by supposing that there are great oscillations, both periodic and occasional, of the surface or crust of the earth, — oscillations which, unlike the abnormal and obstructed ones called earthquakes, are noiseless, far-sweeping, and powerful in their operation, like the ground swell of the ocean. These oscillations produce the horary variations of the barometer, observed by Humboldt near the equator, which give two *maxima* and two *minima* every twenty-four hours; the regular recurrence of these phenomena, he says, being "undisturbed by storms, hurricanes, rain, and earthquakes," though the density of the atmosphere must be greatly affected by agencies so powerful. They produce also the greater, occasional variations of the barometer, which sometimes take place in a calm day with an equable temperature, when there is no indication of disturbance in the atmosphere. During the great earthquake at Lisbon, the mercury in the barometer sank, even in Great Britain, so as to disappear from that portion of the instrument which is usually left uncovered. There have also been observed, on our great lakes, sudden and great retrocessions of the waters at different periods, which were not accompanied by any remarkable phenomena in the atmosphere. Thus, in the summer of 1834, the waters suddenly receded from the outlet of Lake Superior at the Sault St. Marie, leaving the bed of the river, there nearly a mile wide, exposed for the greater part of its breadth, in which state it remained nearly an hour, when the waters came down again in a vast surge. A broad and silent undulation of the crust of the earth alone seems sufficient to explain such a phenomenon.

Returning to the subject of the barometer, the author argues that, even if the atmosphere were of varying density, the construction of the instrument is such that the barometer could not be affected by the variation, and the increase of density cannot be traced to gravitation.

"In the first place, when the barometer is constructed, the mercury in the tube does not owe its elevation to the pressure of the air. Let a tube closed at the top be immersed in mercury, and, when filled, let it be withdrawn upward by the hand; the

mercury is raised with the tube above the level of the mercury from which it is taken. The force required to lift it is just the weight of the mercury and tube together. The pressure of the atmosphere on the lower surface of mercury does not in the least degree aid in the elevation, does not diminish the weight; — because the sustaining mercury is pressed by the air, not one iota less of force is required to give the sustained mercury its elevation. The fact is admitted, and is accounted for in this manner: — 'The force used in lifting the mercury is needed to elevate the air over the tube; that being done by the applied force, it is the weight of the atmosphere which elevates the mercury in the tube.' But this reason is given without reflection; for the air is no heavier over the tube when the tube is filled with mercury, than when it is filled with water or air; yet the force applied measures the weight of the fluid within, and the contained weight does not increase or diminish the weight of the column of air over the tube. It is certain, therefore, that when the column of mercury is lifted with the tube, it is the applied force that lifts it, and not the weight of the atmosphere bearing on the surface of the mercury from which it is withdrawn. Of course, the weight of the atmosphere, not capable in the least of aiding the process of elevation, has no efficacy in sustaining the column when raised. One would almost suppose that an air-supported column of mercury would be like the air-supported balloon, and would not press with any weight upon the instrument." pp. 123, 124.

All are familiar with the experiment of filling a tumbler with water, pressing a thin paper, slightly moistened, over its mouth, and then carefully inverting it, when the water is sustained in the vessel, or kept from falling, solely, as it would seem, by the slight adhesion of the paper to the edges of the glass. The explanation usually offered is, that the pressure of the atmosphere against the paper sustains the water, which cannot fall except by leaving a vacuum in the upper part of the inverted tumbler. But the experiment succeeds equally well, if only two thirds or one half of the tumbler be filled with water; then, on inverting it, the space between the top of the water and the closed end of the glass is filled with air of the same density and elasticity as that which is outside. The upward and downward pressure on the water ought, therefore, to neutralize each other; yet the water remains suspended. Moreover, the *weight* which is sustained by the hand that holds the tumbler at a fixed level is equal

to the weight of the glass and the water taken together; how, then, can the weight of the latter be sustained exclusively by the pressure of the air from below?

"If it were possible that the sustained mercury under any circumstances could select a column of air of its own area, out of a volume of air pressing by elasticity in every direction, and disregard the pressure, weight, and elasticity of other columns which act on the surface of the cistern, yet it would be impossible as the barometer is usually constructed. The cistern is closed at top; its bottom is a piece of leather; the instrument is inclosed in a wooden case and suspended perhaps in a close room. It then receives the pressure or weight of its selected column of air forty-five miles high, one quarter of an inch in diameter, which may be twisted and beat about by the winds of heaven, and reaches the instrument through doors or windows, passing through the air in the room, which is subject to expansion and condensation by artificial changes of heat. Yet, not affected by these changes, it penetrates through the openings of the wooden case, acts on the bottom of the leathern support, and still gives exactly the degree of pressure belonging to its area, to the mercury in the tube through an aperture of any size! Is it possible that any thing can be measured by such an instrument, except, perhaps, the elasticity of the air in the apartment in which it stands, which would affect the whole extent of the surface of the mercury on which it acts?

"But if this last and only possible action of the air by its elasticity be the cause of the oscillation, why is it that two upper surfaces of mercury, one in the cistern and one in the tube, are an essential condition of the oscillation? A tube in a conoidal form with a piece of leather over its base, would as well indicate the pressure of the atmosphere. The leather would act as well thus placed. It would have the same elasticity. The air pressing on it would act as truly without an upper surface of mercury as with it. The fact that two upper surfaces of the fluid are necessary, abundantly proves that another cause for the action of the barometer is yet to be discovered." pp. 127, 128.

Thus far we have followed the reasoning of the author with little comment, our object being to give an exposition of his views, not our own mode of refuting them, especially as the former object alone would require more space than we can spare. But here we must say, that one of the objections to the received theory of the barometer, which is just intimated in the last extract, and which is expanded and strongly urged elsewhere, is not only fallacious, but is inconsistent

with another portion of the writer's own doctrine. Mr. Coues cannot see why, on the received theory, the column of mercury should be sustained only by a column of air of its own area, and should not feel the effect of the much larger column that rests on the whole surface of the mercury in the cistern; for he says truly, "it makes no difference in the action of the barometer whether the surface of the cistern is of two inches area, bearing a column of air of two inches area, or is an ocean of mercury, bearing the atmosphere of a hemisphere."

But, "fill a cylinder with mercury, and from the cylinder let a long tube ascend; subject the mercury in the cylinder to the pressure of a piston, and the rise of the mercury in this tube,—that is, the weight of the mercury supported by the pressure of the piston, — will be measured by the whole pressure of the piston, not by its pressure on a surface of mercury in the cylinder equal merely to the area of the tube in which the mercury is supported."

We doubt the fact. Suppose the cylinder to be ten square inches in area on a cross section, and the tube to be but one inch in area; then, if a piston nicely fitting the cylinder, and with an orifice in it to admit the tube to pass through, be pressed down upon the upper surface of the mercury with a force of one hundred pounds, only ten pounds of mercury will rise above the level of the piston into the tube, which we suppose to be open at the top. It must be so; ten pounds at a higher level in the narrow column balance one hundred pounds at a lower level in the wider column; for if the upper surface of the former descend ten inches, the upper surface of the latter will be raised but one inch. Mr. Coues himself recognizes this fact, and explains it, when treating of the hydrostatic paradox and the Bramah press. He says very truly, "the fact that the weight or pressure of one pound of water may be made to produce a pressure equal to that of a hundred or a thousand pounds, is in reality no more paradoxical than that one pound on the long arm of a lever should balance a greater weight on the short arm." Is it not evident that the same principle applies in the case of the barometer? If the mercury in the cistern present an area of upper surface ten times larger than that of the mercury in the tube, a descent of ten inches in the tube will

raise the surface in the cistern only one inch. Make the surface in the cistern ten times larger, and the same descent in the tube will raise this surface only one tenth of an inch; make the cistern as large as an ocean, (to adopt our author's illustration,) and this descent will raise the surface by a quantity almost infinitely small.

Mr. Coues forgets the hydrostatic paradox and his explanation of it, not only when treating of the barometer, but when he comes to speak of the buoyancy or floating of bodies. He explains the paradox, by saying that "the molecular force due to the greater depth of the long column is diffused through the less depth of the water in the cylinder; and being equally diffused through this body of water, its action is of course in proportion to the area of the water." But he afterwards says, "the force of molecular action will *not* account for the buoyancy or floating of bodies." He affirms, also, that buoyancy is not produced by the gravitating power, because "bodies will float in water of less weight than themselves." If one cup be placed inside of another, which is but little larger, so that their surfaces are nearly in contact, and water be then poured between them, the inner cup will be lifted, and will float, long before water enough is poured in to equal its own weight; and a ship placed in a repairing dock, which it very nearly fills, does not require water enough to equal its own weight before it will float. Very well; the tall and thin sheet of water between the cups, or between the sides of the dock and those of the ship, answers exactly to the long and slender column of water in the hydrostatic paradox and in Bramah's press; its pressure is multiplied as many times as the area, or cross section, of the floating part of the ship or inner cup exceeds in size the area of the thin sheet of water at the sides.

Mr. Coues farther objects, that the loss of weight by a submerged body, a loss equal to the weight of that volume of the fluid which it displaces, cannot be ascribed to the pressure of the fluid, because that pressure is equal in all directions,— upwards, downwards, and sidewise,— and these equal opposite pressures must cancel each other. He forgets that the upward and downward pressures are equal only when they are measured from one and the same point; and that the submerged body has bulk, which prevents these pressures

from being thus measured. The side pressures, being opposite and equal, do indeed cancel each other; but not so with the vertical pressures. Suppose the submerged body, a block of wood, is six inches thick, and that its upper surface is six inches below the surface of the water; then that upper surface is pressed *down* by a column of water of its own area *six* inches high, while the under surface of the block is pressed *up* by a column of water of the same area *twelve* inches high. Of course, the block must lose in weight a quantity just equal to the weight of the water which it displaces, or which is equal to its own bulk.

In speaking of the Magdeburg hemispheres, Mr. Coues seems to forget the very principle on which he here dwells at length, — that opposite pressures cancel each other. These hemispheres are two hollow half globes of metal, so fitted to each other that their lips, when touching, make an air-tight juncture. The air within can be exhausted by an air-pump, and the pressure of the external air then binds the hemispheres together with a force equal, as is usually said, to "as many times fifteen pounds as there are square inches *in the area of the mouth*, or at the surface of the division of the upper and lower hemispheres." Then the binding force, says Mr. Coues, cannot be the pressure of the atmosphere; for if so, "the external pressure would be in proportion to the surface exposed, — to the surface area of the sphere." Surely, the side pressures, being equal, must be borne by the strength of the metal, which prevents the sphere from being crushed in at the sides. Only that part of the atmospheric pressure can bind the half spheres together which operates perpendicularly against the area at the plane of junction. The shape of the instrument does not affect the result; two rectangular boxes of metal, each open only at one end, will answer as well as two hemispheres; and in the case of two such boxes, we can see clearly that the side pressures neutralize each other, so far as the adherence of the two halves to each other is concerned.

But the following objection to the received doctrine about the Magdeburg hemispheres is more acute, and less easy to be explained away: —

"It is said that, when these hemispheres were first exhibited, the inventor had a pair made of a foot in diameter, and that six

horses were unable to pull them asunder. Suppose the metal to be thicker, so that the vacuum would be only half the size. We know that much less force would be required to pull them apart, and yet the weight of the atmosphere around them would remain unchanged." p. 204.

We may ask further, — suppose the two hemispheres to be solid, or not hollowed at all; why should not the pressure of the atmosphere unite them just as strongly as if they enclosed a vacuum? Or suppose any two flat pieces of metal, with surfaces perfectly plane, so that, when united, no air could enter or remain between them; why does not the pressure of the atmosphere offer just as much resistance to the separation of these, as of the Magdeburg hemispheres?

We have not space to dwell upon the author's consideration of the theory of the tides, the trade winds, and many other phenomena, in relation to which we think he has been more successful than in treating of the pressure of fluids. Indeed, our limitation of room has caused us to do him serious injustice, even on those points which we have treated most at length; for to the reader who depends on our imperfect sketch alone, the doctrine will seem open to many serious objections, which he might find fully considered, and perhaps satisfactorily answered, in the work itself. We can, therefore, only commend the book strongly to lovers of science, as the production of a vigorous and original thinker, who is able to present his thoughts in perspicuous and graceful language, and to surround them with pertinent and attractive illustrations. They may not accept the conclusions of the writer; but they cannot fail to be pleased with the tendency of his doctrine, and with the manner in which it is developed and explained.